rently do not sleep at all. The leaves of *Euphorbia jacquiniæflora* depend vertically at night, whereas those of a dwarfish Brazilian species rise vertically up at night. The leaves of this Euphorbia stand opposite one another—a position which is rather rare in the genus; and the rising movement may be of service to the plant, as the upper surfaces of the opposite leaves mutually protect one another by coming into contact. In the genus Sida the leaves of two species rise, while those of a third Brazilian species sink vertically down at night. Two species of Desmodium are common plants near Fritz Müller's house: in one the leaflets move simply downwards at night; but in the other not only do the three leaflets move vertically down, while the main petiole rises vertically up, as is likewise the case with *D. gyrans*, but in addition the lateral leaflets rotate so as to stand parallel with the terminal leaflet, behind which they are more or less completely hidden. This, as far as I have seen, is a new kind of nyctitropic movement; but it leads to a result common to several species, namely, that of packing the three leaflets closely together and placing them in a vertical position.

Down, Beckenham, Kent, April 14 CHARLES DARWIN

Spectrum of the Star Ll. 13412

THE spectrum of the star Ll. 13412 appears to be in some respects unique. It consists mainly of three bright lines having wave-lengths of 545, 486, and 466 millionths of a millimetre. Four other stars have hitherto been found whose spectra are of this character. Three of them are in Cygnus, and have lines whose wave-lengths are 580, 568, 536, and 467. The fourth star, Oeltzen 17681, has lines at about 582 and 470 (NATURE, vol. xxii. p. 483). The line or band at 467 appears to be common to all, and that at 580 to the last four. The line at 486 in Ll. 13412 coincides with the F line of hydrogen, but is not visible in the other stars. The line at 545 is also absent in them. This star therefore appears to resemble the others in kind, but not in the material of which it is composed. It is also much brighter than the others, so that it is not a difficult object with a small telescope. Its position for 1880 is in R.A. 6h. 49·3m. and Dec. – 23° 47′. It is easily found as a seventh magnitude star about 15′ north of o′ *Canis Majoris*.

Cambridge, U.S., April 14 EDWARD C. PICKERING

The Indian Winter Rains

IN NATURE, vol. xxiii. p. 400, Mr. F. Chambers very properly points out that the winter rains of Northern India, though usually heaviest in years when the *mean* pressure is above the average, are yet coincident with short periods of low rather than of high pressure. The way in which Mr. Chambers accounts for the low pressure seems, however, rather far-fetched. It is true that on one or two of the American weather charts storm tracks are shown extending from the Mediterranean to Northern India or the Bay of Bengal, but these paths are drawn with dotted lines indicating that they are doubtful, and, considering the absence of meteorological stations in the greater part of the area between the Mediterranean and India, and the nature of the intervening country—especially Afghanistan with its high mountains—I should say the evidence upon the strength of which the American chartographer laid down these storm tracks, was of the slightest possible description. The winter rains are however accompanied by a cyclonic movement of the air over Northern India, and I wish to point out that, whether the cyclonic disturbance be a European or Transatlantic visitor, as Mr. Chambers supposes, or a native of the Indian region, generated by the rainfall, as Mr. Eliot has taught in his report for 1877, the "old notion" of the connection of the rains with the upper anti-monsoon current is by no means exploded. The progress of the disturbance and of the rainfall is usually from north-west to south-east, and the rainfall is heaviest, as a rule, on the eastern side of the disturbance. The winds which bring the rainfall therefore come from some southerly quarter, and as northerly winds generally continue blowing in the extreme south of India at the time when these disturbances occur in the north, the southerly rainy winds must be derived from an upper current which descends in the anti-cyclone or region of high pressure in the centre and south of India, or in the zone between the south of India and the equator. Mr. Blanford's modification of his former views regarding the origin of these rains appears from his remarks and the accompanying charts in the Meteorological Report for 1878 to be merely that the indraught towards the region of precipitation is not confined to Northern India, but is occasionally, though rarely, felt as far south as Ceylon.

In a letter of mine that appeared in NATURE for the following week (p. 409), there was a mistaken inference from Mr. Blanford's investigation regarding the "Barometric See-saw" between India and West Siberia that I beg your permission to correct. The mean pressure at sea-level in the Indo-Malayan and West Siberian regions appears from Mr. Buchan's charts to be nearly the same both on the average of the year and in January and July. Also no wind blows directly from the one region to the other. We cannot therefore infer anything regarding the strength of the winds from Mr. Blanford's results, but we may regarding temperature. The proper inferences of this kind from the results arrived at by Mr. Blanford and Mr. Archibald appear to be these:—(1) The range of temperature in the 11-year period is greater in Siberia than in surrounding countries; (2) Siberia is coldest, compared with neighbouring countries, at times of maximum sun-spot; (3) This relation is most marked in winter; and (4) near the coasts of the Pacific (Nertchinsk, Pekin, Zi-ka-wei), the Indian (all the Indo-Malayan stations, especially those nearest the sea), and the Atlantic (London) oceans, where presumably the range of temperature is less than in the heart of the continent, the variation of the barometer in the 11-year period is opposite to that observed in Siberia. S. A. HILL

Allahabad, March 29

Palæolithic Man

IT is desirable that further search should be made for implements made by man in the deposits of this country assumed to be older than the well-known and accepted implementiferous river-gravels.

In the gravels belonging to the Thames, in and near London, palæolithic implements are of not infrequent occurrence. In my own collection I have more than 120 examples—with few exceptions found by myself—and I know of at least another hundred specimens found chiefly by London friends who have availed themselves of hints given by me.

My object now is to direct attention to the fact that the implements are not only found in and near London in the lower and middle terraces of gravel some 25 to 70 feet above the ordnance datum, but at far greater heights. Some of these heights near London may, and others no doubt do, belong to the Thames or to its tributaries, but they all (in different degrees) appear to point to a more remote time than the period when the lower terraces of the Thames and its tributaries were formed. Some of the implements now found in the lower gravels are clearly "derived" from more ancient deposits. For instance, I have one example white in colour and highly porcellaneous:—the white colour has been brought about by the decomposition of the flint in some ancient loam or clay, and not from the gravel in which the implement was found: this is proved by several more or less highly-polished accidental fractures at the edges of a different colour from the general white surface: these coloured fractures are more recent than the white facets, and date from the last deposition of the implement in the lower terrace: the white abraded flakings belong to a highly remote time. Dr. John Evans records the finding of an implement in the Thames gravel at Highbury, at 102 feet, whilst I have found one (also near Highbury), at an elevation of 144 feet. Last summer I found an implement on the eminence at the north of, and overlooking Ealing Dean, at a height of 164 feet. This is 72 feet higher than the implementiferous beds of Ealing Dean described by General Pitt Rivers, and between 80 and 100 feet higher (in one instance 104 feet higher) than the implement-bearing gravels at Acton described by the same gentleman. The gravel at the 164-feet elevation forms an isolated patch on the extreme top of a hill. I watched the excavations here (which were shallow) for road-making, with great care, and with the implement I found several flakes. These heights agree well with the heights of some of the implementiferous gravels found capping the cliffs in the South of England, also with the Erith position at Northumberland Heath, where Mr. F. C. J. Spurrell found an implement at an elevation of 175 feet.

Most geologists know the high gravels overlooking Hertford, Ware, and Amwell; their altitude is from 130 to over 180 feet above the ordnance datum. Gravel from the two first of these places is brought to London for ballast in thousands of tons. A year or two ago great quantities of gravel from Hertford were brought to Finsbury Park by the Great Northern Railway, and in the gravel thrown down near Finsbury Park I

© 1881 Nature Publishing Group

picked up a good sub-triangular wedge-shaped implement. Further search produced a second implement, a good trimmed flake, and a few simple flakes. The worked flints in the Hertford gravel are however so rare that the search for them is the most hopeless task conceivable. There is not more than one flake in 500 tons, not one implement in 5000 tons of gravel. The gravel from Ware is also brought to the east of London for ballast, and I happened last year to mention the fact of my discoveries to Mr. J. E. Greenhill, the Principal of a school near Hackney Downs. Mr. Greenhill at once not only searched himself, but set his pupils to look over the Ware gravel, then laid down in large quantities near Clapton, with the result that a large broken ovate implement was found and several flakes. I also found a large and heavy "slice" flake with numerous facets on its worked side in the same gravel. Mr. Greenhill's success caused me to look carefully over a similar lot of gravel from Ware, laid down near Victoria Park. In this I found a sub-triangular implement and three flakes. I have also found a large greatly abraded flake in the Amwell gravel at Amwell. Elsewhere in east and north-east London I have looked over thousands of tons of Hertford and Ware gravel without decisive result. A week or two ago, however, as my younger son was returning home through Finsbury Park, he picked up a good scraper-like implement in the gravel (presumably from Hertford), just thrown on to the road inside the park. On hearing of the discovery I at once went to Finsbury Park and looked carefully over all the recently thrown down material, but with no further result. I have visited the different pits at Hertford, Ware, and Amwell several times, but there is never enough gravel exposed (considering the extreme comparative rarity of the implements and flakes) to give one a chance of finding an implement. I have found in the pits several simple flakes, with the cone of percussion, and that is all. At what depth the implements occur in the gravel I do not know, but that implements really do come out of the high gravels overlooking Hertford and Ware I think there can be no doubt. Reference was made by me to these implements at the Anthropological Institute three years ago, when two or three specimens were exhibited by me.

WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

Sound of the Aurora

WITH every respect for the ability and acuteness of the late Sir John Franklin and his companions, I do not think it conclusive, as Mr. Rouse seems to do, that because they heard no sounds "with the aurora borealis" (NATURE, vol. xxiii. p. 556), no sounds are produced by it.

All Indians, both on the shores of Hudson's Bay and near Bear Lake, and the Eskimos on many parts of the coast, assert positively that the bright, varying, flickering,[1] and rapidly-moving auroræ do produce sound. The senses of hearing and smelling in the Indian and Eskimo are far more acute than in the civilised man; and both sounds and smells which to the latter are not perceptible are perfectly so to the more sensitive auditory and olfactory organs of the savage.

The theory that "the attractive force of the aurora is increased within a certain limit as its rays proceed southward" is scarcely borne out by my experience.

When wintering at Fort Hope, Repulse Bay, in 1846-47 and 1853-54, lat. 66° 32′ N., the result of my observations was, as far as I can discover, exactly similar to that of Parry in 1824-25 at Port Bowen in lat. 73° 15′ N., 400 miles further north and fifty miles west of Fort Hope: at both no effect was produced on the magnetic needle.

At Repulse Bay, and it may have been the same at Port Bowen, the character of the aurora was perfectly different from that generally seen at Great Bear Lake, which acted so powerfully on the needle, the former being almost always of a uniform pale yellow or straw colour, with little rapid motion, whereas the latter was generally flashing, flickering, rapidly moving, and of diverse hues.

One peculiarity of the auroræ observed at Repulse Bay may be worthy of notice: they were chiefly seen to the magnetic south—that is south 62° east true—usually in the form of an arch rather low down—and I may add that in that direction at a distance of thirty or forty miles from our head-quarters a large extent of sea is kept open all winter by strong currents. The Eskimos of Repulse Bay do not say much about the aurora beyond expressing a belief that it is the spirits of their dead visiting each other in the heavens.

It is probably a matter of little or no importance in a question of this kind, but Mr. Rouse has given the latitudes of the southern shores of Great Bear Lake from 90 to 200 miles too far north.

Fort Franklin, where Franklin made his chief observations, is situated in latitude 65° 12′ N. at the extreme south-west of Great Bear Lake, whereas Fort Confidence, where Sir J. Richardson and I made ours with like results, is at the extreme north-east of the lake in lat. 66° 54′ N., the stations being 150 miles distant from each other.

It is perhaps not being too sanguine to hope that in this period of marvellous discoveries, some instrument may be—if not already—invented, with the aid of which one may be able to decide the question satisfactorily as to whether the aurora in any form does or does not produce sound.

JOHN RAE

4, Addison Gardens, April 16

THE SCIENTIFIC PRINCIPLES INVOLVED IN ELECTRIC LIGHTING[1]

II.

Lectures III. and IV.

ALL machines for the conversion of mechanical work into electricity are founded on Faraday's great discovery of the induced current derived from the relative motion of a magnet and a coil of wire. They are either continuous-current or alternate-current machines. From the continuous-current machines of Pixii in 1832 and Saxton and Clarke in 1835 and 1836, we pass to Wheatstone's introduction in 1845 of electromagnets in place of permanent magnets to produce the magnetic field. In 1854 Werner Siemens and Halske introduced the Siemens armature, in which the coil is wound longitudinally in a groove. In 1854 Hjorth patented an improved magneto-electric battery, in which the currents induced in the revolving armature pass round the electromagnets and produce the magnetic field. This is the principle of the dynamo-electric machine, which was afterwards rediscovered by Siemens and by Wheatstone simultaneously in 1867, when on the same evening their two papers were presented to the Royal Society.

Then followed the Gramme armature, in which coils of wire are wound in sections all in the same direction round a ring; each section, when a current is flowing through it, may be regarded as an electro-magnet, and its principle of action is clear at once from the principles of Arago and from Lenz's laws for induced currents.

In dynamo-electric machines the external work in the electric are is proportional to the square of the current, and is also proportional to the number of turns of the armature per minute.

Any disturbance in the resistance of the arc reacts on the electro-magnet, altering the strength of the magnetic field, thereby increasing the disturbance; this is the great disadvantage of dynamo-electric machines as compared with magneto-electric machines, where the magnet is either a permanent magnet or is excited by means of a separate current. Wilde, in 1863, employed a separate continuous current machine to give a permanent magnetic field, and made the armature of the second machine to revolve in this magnetic field. In alternate-current machines there is no commutator for making the current continuous, but the currents from the coil are collected and sent through the external resistance in opposite directions for every half-turn of the armature. The Alliance magneto-electric machine was the first of these, which was converted by Holmes into a continuous-current machine, and was by him first used in 1858 to produce the electric light for lighthouse illumination. He afterwards again converted his machine into an alternate-

---

[1] I borrow this most appropriate term from Prof. Stokes, F.R.S., &c., of Cambridge.

[1] By Prof. W Grylls Adams, F.R.S. Continued from p. 582.

© 1881 Nature Publishing Group